however, by a knowledge of the laws of physiology and autopsic inspection, we did not expect to find the author straightway propounding a theory of his own, which, we fear, will be found to rest on no better data than he had the page before deprecated. This theory is no further novel than in the extensive application made of it to explain the production of the multiform characters of disease, but as it constitutes the key-stone of the system of pathology, which this treatise is devoted to verify and enforce, we shall give it in the author's own words, that the reader may be fairly put in possession of the aim and tendencies of the work under consideration.

"The first question which pressed on his mind, related to the cause of the derangement of the functions of the liver in the autumnal diseases. The symptoms observed were very commonly, by the mass of people and by many physicians, attributed to superabundance of bile, and this as well as many other popular notions, was derived from high authority in former days. The obvious relief afforded by a free discharge of bile, gave considerable countenance to the opinion; but the unanswerable objection was, that very often there is no bile discharged either from the stomach or the bowels, by the most active medicines, and the symptoms are more severe than in those cases in which there is an abundance of that fluid—and that a free secretion of bile is a most desirable object. It was evident then that the morbid symptoms are produced not by bile, but by the retained material of which bile is formed, viz. the blood in the liver. This led to the doctrine of congestion in that viscus being the cause of the disorder observed, a doctrine advanced by many late writers.

"It was very evident that this cause is capable of producing an increased secretion of bile, but the question arose, can it likewise produce a suppression of the secretion? From what occurs in the mamma after parturition, there is reason to believe it can. In that gland, a certain degree of congestion or accumulation of blood, produces increased secretion; a higher degree, such as produces hardness, suppresses secretion entirely.

"The next question was, how are the other parts, the stomach, the head, &c. affected? By sympathy, is the common answer; which, while it acknowledges an intimate connexion between these parts, conveys no precise idea of it, and contains no explanation of its nature.

"Revolving in his mind the nature of this connexion, the thought struck him that congestion, or accumulation of blood cannot exist in the liver, without extending into the vena cava, and its great branches, the jugulars, the emulgent veins, and the internal and external iliac veins; nor without producing an accumulation in all the veins leading into the liver, viz. in the veins of all the chylopoietic viscera. It was at once apparent that the same accumulation must affect the brain, and the stomach and bowels; and derange the functions of the latter, at the same time that it deranges those of the liver. Further reflection led to the observation, that the same accumulation affects the kidneys and the uterus also.

"Considerable time was spent in tracing out the effects of this accumulation of blood in the vena cava and its branches, in the several parts in which they are situated.

"It was found that this cause is capable of producing pain in the head, vertigo, stupor, &c.; enlargement of the liver and pain in that part, increased secretion of bile, with nausea, vomiting and purging; or suppressed secretion and constipation; and in the glands of the stomach, increased secretion with ravenous appetite, or suppressed secretion and consequent want of appetite, flatulence and acidity; together with derangement of other parts not necessary to particularize.

"An accumulation of blood in the interior veins was thus found to be capable of producing the symptoms preceding and accompanying fever: it still remained to account for the increased action of the heart and arteries. Blood being the natural stimulus of the heart, it is evident that a sudden increase of the quantity poured into it, must produce increased action of that organ, if it be at the time capable of it. The sudden accumulation therefore which takes place in the cold stage of fever produces the increased action that follows; while the above-mentioned affections of the head, stomach, liver, &c. which precede fever, are the effects of gradual accumulation of that fluid in the vena cava, &c.

"The next question was, what is the cause of this accumulation? It had been observed that weakened action of the heart is always present in the commencement of these autumnal diseases, and that it is produced by all the remote causes of fever; and it was evident that it must, whenever present, necessarily produce accumulation of blood in the vena cava, &c.; and therefore that this is the cause."

The facts and arguments on which this theory rest for support, will be examined as they occur in course. For the present, we shall content ourselves with remarking, that it is much too mechanical for general reception in this age of physiological and vital pathology; and, moreover that it is no where shown that the liver is actually in a state of congestion, or that the vena cava and its branches seriously suffer from an accumulation of blood in the precursory stage of fevers, though it must be admitted that such accumulation to a certain extent constantly takes place during the continuance of rigors, from whatever cause induced; consequently, we are constrained to class this theory, according to the author's own definition, among those hypotheses which have been so long the bane of medicine.

After passing in review some of the more prominent doctrines, from HIPPOCRATES to the time of RUSH, which have exercised a controlling influence over the science, and pointed out what he conceives to be their radical defects, especially their neglect to trace the symptoms of disease through their train of causes up to their remote cause, he proceeds to the consideration of the several remote causes of fever. These he endeavours to prove are all either directly or indirectly debilitating. On this point we would remark, that whatever may be the mode of operation of these causes, it cannot be denied that fever does not take place until a general or local irritation is set

up, which is, indeed, itself a state of fever, whenever the irritation is sufficiently intense to affect the rest of the system. Besides, the view taken of the operation of the remote causes, necessarily supposes fevers to be idiopathic and general affections, a doctrine fast vanishing from the minds of physicians, and which the researches of Broussais, Louis, Andral, and others, particularly the pathological anatomists, render more than problematical. We cannot at this time enter upon the discussion of this interesting and important question, for it would lead us too far away from the subject in hand, and must therefore refer the reader, for a triumphant refutation of the ancient errors on this subject, to Broussais's *Examination* and the *Pyretology* of Boisseau, of which latter work a full analysis was given in the last number of this Journal. We will merely observe in passing, that if it can be shown, as we believe it can be, that the fevers heretofore esteemed idiopathic arise from local irritations, and are nearly allied with phlegmasial affections, that then whatever may be the nature of the remote causes inducing them, that these causes must be either directly or indirectly essentially stimulating in their operation, a conclusion in contravention of the author's theory. Let us, however, examine a little in detail his explanation of the mode of action of some of the more frequent of these causes, in order that we may be enabled to judge how far he has been successful in fortifying his assumed premises.

The first remote cause of fevers noticed, is prolonged abstinence from food, and insufficient nourishment. Now we are free to admit that the first effect of this cause is not only weakened action of the heart, but of all the powers and functions of the system. This state of depression, however, is not a state of fever; on the contrary, it is universally considered one of our most efficient means to remove or alleviate an existing febrile affection. When abstinence is carried to the extent of producing disease, it does so not by weakening the action of the heart, which effect rather wards off for a time the evil, but by changing the character and qualities of the circulatory fluids, and rendering them so acrid and irritating as to produce not only most intense inflammation of the stomach and intestines, but also of other tissues of the system, which, when they prove fatal, destroy life amidst unexampled pain and suffering, as was evinced in the case of the criminal in France, who some years ago destroyed himself by voluntary starvation, and whose case was detailed with great minuteness in the French journals at the time. M. Gaspard, in Magendie's Journal, Vol. I. p. 237, relates the effects of a most desolating famine which occurred in a part of France, in 1817, where the inhabitants

were constrained to divide with the cattle the herbage of the fields, and to satisfy the cravings of hunger with all sorts of vegetable productions that fell into their way. The effects that ensued from this insufficient and gross nutriment were general serous diathesis with hydropic affections, without organic lesions of the chylopoietic viscera, and a remarkable exemption from fevers and febrile affections. These observations, with others of a similar character, that might be quoted, convince us that systematic writers have servilely copied from one another, without examining into its accuracy, that famine and moral causes are the frequent remote causes of fevers. We do not deny that they are sometimes predisposing and perhaps even adjuvant causes, not only of fevers, but of many other diseases, by depressing the vital powers of the system, and thus rendering it more susceptible to morbid derangement; but we think it would puzzle our author to adduce an instance where either of these causes has unaided produced what he would call an idiopathic fever.

The next remote cause mentioned is excessive bodily exertion. Undoubtedly this is often followed by great languor and debility, especially when its effects terminate without further ill consequence. but when fever supervenes from this cause alone, it is usually during the state of excitement and tumultuous hurry of the circulation induced by the over-exertion of the muscular powers. The same remark applies to stimulant drinks as a cause of fevers. We are astonished that every physician's personal experience does not afford him the proof of this position. The constant attendant on taking an undue quantity of stimulating drink, is in fact a state of febrile excitement, which commonly ceases with the dissipation of its cause, and yet it will be hardly contended that that excitement has been preceded by weakened action of the heart.

In making contusions from external violence, as falls and blows, the cause of fevers, the author has drawn his argument from the analogy of their first effects to concussions of the brain from the same causes. Here the same objection nearly recurs that was made when speaking of the effects of starvation. As long as the nerves of the part suffer from the benumbing or paralyzing effects of the external violence, there is no fever induced: when the injured part becomes the seat of irritation and fluxion, then fever is lighted up by this irritation, and not because the nervous system has been temporarily interrupted in its functions. In those cases where the brain is the suffering organ, inasmuch as its functions cannot be long suspended without fatal consequences, we are obliged to rouse the vital actions by the use of stimulants, although we are sensible that the after

effects of the injury will call for the most active depletory measures, and this too in cases where no concussion has been produced.

Cold is one of the most constant and evident causes of disease, and the author endeavours to prove, chiefly from the facts furnished by CURRIE, that it invariably does so by weakening the action of the heart. This position is tenable to a limited extent only. Many facts and other authorities can be adduced to show that its modus operandi is far from being so simple as is here supposed. We have always considered the observations of the late Dr. Currie on the action of this power, as rather popular than profound. His work, indeed, is eminently practical, and in that way has been of great service in pointing out to physicians under what circumstances cold may be resorted to with safety for the reduction of febrile excitement; but other authors have treated of the operations of this agent on the system in a more philosophical manner. The work of M. BEAUPRE on this subject is one of the ablest and most satisfactory that we have met with. His experience, and his opportunities for observation of its effects, which were very extensive, led him to assign to the operation of cold, effects differing greatly, according to the circumstances under which it was applied, its degree of intensity, and the condition of the system at the time. Sometimes, he says, it is refrigerant, sometimes a tonic, in other instances an astringent, then again a sedative like opium, or a pure debilitant; whilst under ordinary circumstances it diminishes sensibility and increases contractility. Its *impression*, an effect overlooked by physicians, he considers to be decidedly stimulant to the living fibre. Dr. Cooke differs entirely from such views. He rejects even the modifying effects of other concurrent causes contended for by Currie, and pronounces its operation to be always directly debilitating, weakening the action of the heart and arteries, and to be indirectly stimulant only by being applied in that degree which is followed by reäction of the circulatory system. The most important circumstance attending the application of cold to the cutaneous surface, is, in our opinion, its revellent effects on the internal organs. In this way it seems to us to produce ordinarily febrile affections, a mode of operation wholly overlooked by our author, and not duly estimated by Beaupré, or any writer with whom we are acquainted.

We think we have said enough to show that the author has not succeeded, to use his own language, "in tracing the remote causes through their chain of effects to the symptoms of disease," by assuming weakened action of the heart, from the nature of these causes, to be an indispensable precursor of all fevers, and we shall not, there-

fore, examine into the operation of the remaining causes with reference to this subject. A more untenable theory, or one leading to more erroneous principles of treatment, we have not of late met with; but the time has gone by when such views can exert much influence over the minds of practical physicians, and we may safely turn it over to the fostering care of the author and his disciples:—

——Velut inter ignes
Luna minores.

The three succeeding chapters on the remote causes of the epidemics of hot climates are very interesting, and may be consulted with advantage by all who are seeking information on this department of the science. The principal circumstances which have attended the production of these fevers, especially in this country, are detailed with great clearness, and fully establish the doctrine that all these fevers are produced by exhalations from vegetable substances undergoing the putrefactive process, from the joint operation of a due degree of heat and moisture. The doctrines of Dr. Ferguson, that malarious exhalations are independent of vegetable putrefaction, and that in fact a paucity of water is one of the conditions of their formation. is ably confuted, chiefly from the doctor's own facts.

The author, in his eagerness to exclude animal matter from any agency in the production of these epidemics, has, we think, passed over too lightly the proofs of the power of exhalations from putrid animal matter to produce febrile diseases. Numerous instances of their agency in this respect are not wanting in the annals of medicine,* and we ought not to reject them because they do not square with our own theoretical preconceptions, but rather endeavour to ascertain their diversity of effects, and in what the infecting material of each consists. This leads us to the author's next chapter, where he attempts to show that malaria is nothing more than carbonic acid gas. This opinion mainly rests on the assumption that like carbonic acid gas, it is a dense (heavy) air, and has the property of changing the blood when respired to a blackish hue; for the other arguments adduced, as its ready absorption by water and lime, its consumption in vegetation and generation by combustion, &c. are altogether gratuitous positions that need not detain us. As for the argument drawn from the similarity of effects of breathing an air highly charged with carbonic acid gas, and the malarious fevers, the analogy is much too loose and unimportant to be of any force. This consideration of the subject, indeed, rather makes against the theory, and if the agents

* See an elaborate paper on this subject in the preceding volume of this Journal.

shall hereafter be proved to be the same in both instances, we must attribute the diversity of results to some peculiarity of attendant circumstances, which we are not enabled at present to appreciate. It by no means necessarily follows that malaria is a heavy gas, because it is more deleterious near the source of its origin than when wafted into the general atmosphere; for this may be owing to the very concentrated state in which it exists before it is diffused abroad, and so greatly diluted, as to become comparatively innoxious. The second argument has more weight with us, without being entirely conclusive. We are gratified to find the experience of Dr. Cooke corroborating the observations of Dr. Stevens, of Santa Cruz, *(See Vol. VII. p. 505, of this Journal,)* that the blood in these fevers is changed to a dark venous or black colour, although the experiments of Dr. Mitchell, *(See his paper on the penetration of fluids in this Journal. Vol. VII. p. 36,)* render it highly probable that this change may be owing merely to a deficiency of oxygenous gas in the air inspired: still the remarks of our author on this subject, for which he could not be indebted to Dr. Stevens, are highly important, and may ultimately lead to a knowledge of the nature of these fevers, and of the malarious exhalations producing them. In the mean time, we must consider this branch of the inquiry to be *sub judice*, and withhold our assent from any theory that is not supported by more conclusive evidence than we have yet met with on this subject. The morbific cause may be carbonic acid gas, it may be carburetted hydrogen, sulphuretted hydrogen, or arise merely from a paucity of oxygen gas in the air respired, or finally from a subtle material that has heretofore eluded the investigations of both physician and chemist.

The chapter on the origin of winter epidemics is one of the most important and original in the work. These epidemics, which are usually a combination of bilious fevers with pneumonic disease, are considered to arise from the joint operation of miasmata and cold, and to be in fact a continuation of the autumnal epidemic modified by change of season. This connection has been casually noticed before by writers, but without being applied to elucidate the cause and character of those malignant complications, so often exhibited in winter epidemics, and our author has rendered a service to medicine by bringing the subject more explicitly under consideration. We do not entirely coincide with him in the way in which the effect is brought about. We cannot conceive how the miasmata of October can operate conjointly with the cold of February as simultaneous causes. But perhaps we do not comprehend him, and he intends to say that the autumnal cause has produced a morbid condition of the system, which is aggravated into disease by the cold and wet of

winter: if so, we can see no reasonable objection to this view of the matter, except that it is insufficient to account for the occurrence of *all* winter epidemics which are characterized by bilious or gastric symptoms. We believe the winter constitution to be sometimes such as to give rise to these complications without the aid of previous derangement.

The identity of the autumnal epidemic diseases is next insisted upon, but as this subject has been ably and learnedly treated by Rush and other writers to our entire satisfaction, and as no new argument or illustration of force is adduced in corroboration of their views, we shall not repeat what is already familiar to our readers. We will remark, however, in passing, that we were somewhat surprised to find their identity urged on the ground that they were all preceded by weakened action of the heart; independent even of the evident absurdity that would flow from the admission of such a principle, yet one of the main objects of the work seems to be to prove that *all* fevers possess this attribute. Lest we should be thought to misrepresent the author on this head, we will let him speak for himself.

"We infer the same from the identity, in all these fevers, of this new cause, weakened action of the heart, produced by the same remote cause. If we infer identity of the ultimate effects from the identity of the remote cause, and the justice of the inference is confirmed by observing that it is so far correct, we more confidently infer their identity from the identity of a cause nearer than the remote cause.

"It may perhaps be objected to this inference, that weakened action of the heart precedes variolous fever also, and therefore the identity of the autumnal fevers cannot be inferred from their being all preceded by this state of the system. It is replied, that weakened action of the heart uniformly produces certain effects called fever. Even when this cause is produced by a peculiar remote cause, the variolous virus, it produces its proper effect, fever, modified by certain effects peculiar to the remote cause. As weakened action of the heart, produced by *this* remote cause, uniformly produces corresponding effects, a fever sui generis; so, when produced by another remote cause, miasmata, we infer it will as uniformly produce a corresponding disease. Thus, as the confluent and distinct small-pox, proceeding from one remote cause, the mildest following inoculation with virus procured from a case the most malignant, are one disease; so, fevers, the mildest and most malignant, proceeding from one remote cause, miasmata, are also one."

The remaining part of the first volume is occupied in treating of contagion as a remote cause of yellow fever, plague and typhus, of the origin of plague and of typhus, and of the identity of these diseases with malarious fevers. This part of the work contains little particularly worthy of remark. The same facts and arguments are reiterated, which have been many times before repeated, to show that none of these diseases are propagated by contagion, and that they are

all merely different grades of the same disease, arising from miasmatic exhalations, modified by attendant circumstances. We must enter our caveat against such sweeping conclusions. Ultraism in medicine is as pernicious as in politics, and has injured the best of causes. No American physician knows enough of plague to be able to lay down, with that certainty which science demands, the laws which controul its origin and propagation. On this subject, he must be content to receive his knowledge from the report of others; and every practical physician, we should think, must at times have had his doubts about the contagious character of typhus. At any rate, the exhalations which produce this latter disease are generated under very peculiar circumstances, and are not to be confounded with marsh miasmata, any more than the time of its prevalence is to be identified with the ordinary occurrences of malignant bilious fevers. Had the distinction pointed out and inculcated by Miller, *(See the appendix to his edition of Thomas's Practice,)* been attended to, much of the difficulty which pervades this subject would have been explained away, without resorting to the doctrine of the unity of fevers: a doctrine which we could wish, for the reputation of Rush, had never received the sanction of his great name; and which here, as on all occasions, proves either too much or nothing. The author moreover elsewhere admits, as indeed every physician must, that fevers arise from a diversity of causes. As for the autumnal epidemics of our country, the proofs of their miasmatic origin, and non-contagious character, are ample even to the risk of overlaying the subject; but epidemic typhus, and no fever that is not epidemic is strictly entitled to the appellation, has heretofore been so extremely rare in this country, and some of the circumstances of its propagation are so obscure and puzzling, that we cannot consider the question of its communicability as definitely settled. On the whole, therefore, we hold it to be the wiser course to lay down as positive only what our own experience, aided by that of others, warrants, and to leave to future investigators, more fortunately placed than ourselves, the task of elucidating such other points as fall within their actual observation. Cuvier has somewhere observed that the human mind supports doubts with difficulty, but it is precisely on that account that the learning to bear with them ought to be one of the principal studies of men of true learning. Had our author been impressed with this sentiment, he would have abated much of the confident air with which he advances and maintains his theoretical doctrines on this point, and his readers been spared much that we have passed over in silence.

The second volume opens with an exposition of the effects of

weakened action of the heart, which is considered, as we have seen, the immediate and invariable consequence of the operation of all the remote causes of febrile affections, and indeed of nearly all the diseases incident to the human frame. This weakened action of the heart, aided in some degree by the check given to all the secretions and excretions, is believed to produce an undue accumulation of blood in the venous cavity, (a term employed to denote the vena cava and its abdominal, thoracic, and cerebral branches, which are destitute of valvular apparatus;) and the different symptoms that occur in the course of diseases from this cause, is traced to this venous congestion. Anatomical considerations are urged in proof of the great liability of the venous cavity to become congested with blood from diminished force of the heart's action. This consequence is attributed in a great measure to these veins being destitute of valves to aid in propelling the blood and to prevent its regurgitation, whilst the external veins are guarded against such accidents, by being furnished with them throughout their extent, to accomplish these purposes. Writers, it is true, generally assign this double function to the venous valves, but we were never able to conceive how they could act otherwise than as passive agents, to prevent the reflux of blood from extraneous causes. Hence, those parts only are provided with them, where such causes can operate, as in the extremities, and they are wholly wanting in the exteroal parts of the head, where no liability to such causes exists, and in the tri-splanchnic cavities, except the heart, muscles of respiration, &c. Could internal venous congestions take place as easily as the author supposes, the human system would scarcely ever be without them. Even continued bodily exertion, a little severe, as it usnally excites greater action proportionably in the voluntary muscles than in the heart, must constantly tend to push on the blood into the internal cavities, and thus give rise to undue accumulations in them, were such a cause susceptible of doing it, and was not counteracted by a law presently to be noticed. A reference to the experiments of Barry and Magendie, on the powers of the circulation, might also be adduced, to show that weakened action of the heart alone is incompetent to produce the effect here assigned to it; but we believe the position to be abundantly refuted by the ordinary occurrences of disease. Still it cannot be denied, that in all irritations of internal organs, there is from the very commencement of the derangement, an undue accumulation of blood in the affected organs, attended, in irritations of a certain degree of intensity and importance, with a marked deficiency of that fluid in other parts; this, however, does not arise from the me-

chanical cause supposed, but from a vital law of the economy announced by Hippocrates, in the memorable words, *ubi stimulus, ibi fluxus.* The recognition of this law enables us to explain why it is that the blood deserts the external surface in morbid derangements of internal parts, without having recourse to weakened power of the heart's action, or even to the suppression of the secretions and excretions. The effect of this latter cause to produce congestion, most at best be to a very limited extent; for if, during the first or atonic stage of fevers, there is little waste or evacuation of fluids, there is also little ingesta of any sort taken. Thirst comes on only on the accession of the hot stage, when there is more indication of arterial than of venous congestion.

Having thus pointed out the manner in which he believes this venous congestion to be brought about, the author proceeds to enumerate the different symptoms that are its immediate consequence, mainly relying on the attendant circumstances of feeble pulse, and paleness and shrinking of the external parts, as indicative of this condition. The principal symptoms detailed, as arising from congestion of the venous cavity, are pulsation in epigastrio; palpitations; tumour, pain, heat, and increased sensibility in the abdominal region; oppressed, disordered respiration; head-ache, delirium, convulsions, and coma; serous effusions; hemorrhages; diminished and increased secretions and excretions; rigors, anorexia, nausea, and indeed all the symptoms that usher in a febrile paroxysm, as well as many of those which constitute its stage of excitement. A single remark will suffice to meet this part of our author's theory, and which we are convinced, every physician the least acquainted with disease at the bed-side, will coïncide with us in holding as incontrovertible. It is this. Nearly all these symptoms are commonly attended with a perturbed condition, and undue action of the heart and arteries, and that mere venous accumulation is not sufficient to account for any of them, under all the circumstances in which they are known to occur. Moreover, if the case were otherwise, and feeble pulse and pale skin were admitted to be the usual concomitants of these symptoms, it is begging the question to assume them as indications of venous congestion, until the congestion was first shown to exist, and to be capable of producing such effects. What proofs does pathological anatomy afford of this venous congestion in febrile affections? Absolutely none whatever. A reference to the works of Louis and Andral, the ablest and best authorities on a question of this sort, will show that other and more permanent alterations of structure, occur in the great majority of cases, to account for the

febrile phenomena. But post mortem examinations are not allowed to disturb our author's reflections in pursuing a favourite object, or he would not consider pain, soreness, and heat of the abdomen, to arise from accumulation of venous blood, because they are attended with feeble pulse, and pale and cold skin. He assuredly cannot be ignorant that cases of peritonitis of the most intense grades of violence, every now and then occur, in which the pulse is rendered extremely small and feeble, scarcely to be felt, the skin cold and clammy, the external parts shrunken and bloodless, and all the vital energies of the system seeming to be concentrated in the abdominal region, without being able to excite those sympathetic symptoms which less severe inflammations exhibit. Should a practitioner under such circumstances, judging as our author did in a similar case, (see paragraph 1339,) that the symptoms arose from an accumulation of venous blood in the abdomen, give stimulant remedies, to rouse up the heart's action, the most disastrous consequences might ensue, and lead to a fatal termination of the disease. Nor is this the only instance in which inflammation may be mistaken for congestion. Nearly all diseases are made to consist in accumulation of venous blood, causing obstruction to the blood's circulation. External inflammations, derangements of the nervous system, and the morbid condition of the digestive functions, &c. are considered as depending on the same state, thus overlooking, in a great measure, irritation of the living fibre, the active agent of disease, in favour of a mechanical and passive cause.

As most diseases are considered to be the consequence of accumulation of blood in the venous cavity, from the operation of remote causes weakening the action of the heart, they are divided in conformity with these views and comprehended under the following heads. 1st. Diseases attended with increased action of the heart without local affection, as simple fevers. 2d. Diseases without either increased action of the heart, or local affection, as dyspepsia. 3d. Diseases with increased action of the heart and local affection, as pleurisy. 4th. Diseases without increased action of the heart and with local affection, as the consequences of a bruise, or fall, without an attendant fever. We do not see any practical advantage to be derived from this arrangement, which must besides vary in its application with the theoretical views of each that shall adopt it, and for ourselves, we confess that we are unable to form any precise idea of a catalogue of diseases arranged in conformity with it. We shall therefore pass on to the consideration of the therapeutical department of the work.

*Therapeutics.*—The principles of treatment for all diseases arising from an accumulation of blood in the venous cavity, are comprised in four general indications. 1st. To remove the remote causes operating on the heart. 2d. To excite the weakened action of the heart and support its action. 3d. To reduce the quantity of blood accumulated in the venous cavity. And 4th. To reduce the action of the heart in the stage of excitement. The mode of attaioing the first is pointed out by the nature of the remote causes; the second may be accomplished in febrile affections, by warm drinks, warm applications exteroally, and the exhibition of emetics to throw the blood on the surface, and in intermittents, during the apyrexial period, by giving permanent stimulants and tonics. The author, however, prefers in these latter diseases, as well as in chronic affectioos, the repeated use of cathartic medicines, as calomel, aloes, rhubarb, jalap, scammony, &c. given duriog the time of the lowest stage of weakened action, in order to keep up and stimulate the heart's action, and to produce their evaeuant effects in the after stage of excitement. The cold bath may be resorted to with the same intention; the reäction induced being often sufficient to enable the heart to throw off its accumulated load. The third indication may be fulfilled by the subtraction of blood, but here also the chief dependance is placed on the free and daily use of the cathartic medicines above enumerated, given for their chylagogue effects, so as to produce full, consistent, dark-coloured or bilious discharges, and repeated to the entire removal of the diseases. The fourth indication is met by bleeding, purging, and cold applicatioos. Little reliance is placed on promoting discharges from any other organs than the chylopoietic, and nauseating medicines are rejected, although admitted to be of considerable efficacy, because they are disagreeable in their operation. Such are the therapeutical principles, and if the pathological views are, as we have deemed them, hypothetical, the consequences of these practical precepts will be found we fear, to be something more than imaginary. This constant resort to reïterated purgatiun, not to be restrained even after it has *induced bloody discharges*, cannot be otherwise than disastrous; sometimes immediately so, by aggravating the already irritated state of the primæ viæ; and in other instances, laying the foundation of future ailments, by the production of chronic derangements. This purging is quite a passion with our author, and employed in nearly all occasions, and to answer opposite and contrary indications. To check menstruation when too profuse and to bring it on when suppressed or scanty. He never fears any untoward effects from the most active and stimulating materials

nf this class of remedies, but gives them with the same confidence in uterine hæmorrhages in the latter months of pregnancy, and in hæmorrhages of the intestinal canal, as others would employ them to relieve an impacted state of the bowels. It would seem that in his view hardly any other medicinal effect is capable of removing disease. Has the exhibition of cinchona or the nitrate of silver cured a case of dyspepsia, it was by acting as a purgative. Has tansy or lime water warded off the gout, or calomel and squills removed a dropsy, it was still by their purgative qualities without reference to their operation on the other emunctories of the system.

It is unnecessary to enter into details on the pathology and treatment of individual diseases. They are of course in conformity with the general principles: accumulation of blood in the venous cavity, the all-sufficient cause, and for treatment, blood-letting to relieve this accumulation, when the strength and fullness of the pulse will warrant its employment; but in general the great reliance is on purgation, repeated again and again without looking to any ill consequences that such a persevering course may induce. The infatuation with which the use of pills of aloes, jalap and calomel, is persisted in day after day, till the patient has taken in a case of dyspepsia, not grains, but ounces, and we might almost say, pounds, is really incredible and consternating.*

Well may the author observe that his treatment is, as far as he knows, new! Hamilton, and every writer with whom we are acquainted, are mere slop doctors, compared to him. Even the famous Leroy, of purging memory, (*see his Médecin curatif,*) must quail before him, and wonder how the stomach and intestines of our western brethren can withstand such rough treatment. The extent to which this preference for purgatives is carried may be judged of by the fact, that the repeated use of cathartics is preferred to quinine in the treatment of intermittents, and their daily use persevered in, in continued fevers, in despite of intestinal hæmorrhages, to the entire extinction of the disease.

However the late writers on fevers may differ concerning their seat and local character, all who have made pathological anatomy the basis of their investigations, and the opinion of none others deserve weight in settling these questions, accord to the digestive tube an important agency in the production of the febrile phenomena, or a

* In our periscope, under the head of American Intelligence, we have inserted two cases of dyspepsia related by our author. They afford a specimen of his purgative practice in that disease.

morbid condition of some part of it, which has supervened during the course of the disease. We must be regardless of these conclusions, not to be invalidated by hypothetical reasonings, as well as the happy effects which we have witnessed from the adoption of such views in the treatment of fevers, before we can be brought to consider the combined application of such stimulants as calomel, aloes and jalap, to an already irritated part, can be void of danger, and to be preferred to a soothing ab-irritant plan of treatment. The influence which pathological anatomy now happily exercises over the minds of physicians will not permit them to retrograde to the ancient routine of stimulant treatment, and we do not apprehend that such practical precepts as are inculcated by our author can long stand their ground, even within the sphere of his influence against the advancing triumphants of actual science.

We have given enough of the contents of these volumes to enable our readers to estimate their general character and value. The same zeal and undoubting confidence in advancing novel doctrines pervades every part of them. All is plain to our author, even the obscurest questions in physiology. The arteries are made to terminate directly into veins, in some instances with sensible motion. The glands are nothing more than continuations of arteries into veins, giving rise to excretory capillaries exactly at their point of junction. What has been taken for fibrous structure of the brain is simply medullary tubes for the conveyance of the excretory nervous fluid into the nerves, which are equally of tubular structure. He may rest assured that such doctrines are not now to be received on the *ipse dixit* of any one. They must be tested by long and patient investigation and comparison with all the known phenomena of vital action before they can merit to be thought more than mere conjectures. The science of medicine has within these last twenty years undergone great and salutary changes. Hypothetical reasonings have given place to facts rigidly deduced from experiment and observation; but this change seems not to have affected in any degree the work before us, which is essentially a production of the last century; with the same proneness to theorize, and to rest, for the support of particular views, on the authority of great names, which characterise the productions of those times. This similarity is not, we suspect, accidental. At any rate we have no where the least intimation that the author is acquainted with the labours of Parry, Armstrong, and Abercrombie, of Britain, or of those of Louis, Andral and Laennec, of France, with a host of others, who have

contributed to base medical science on principles unknown to former times.

In closing our observations we must be permitted to remark, that we have never performed our critical labours with greater reluctance than on the present occasion. We have found so much to dissent from and to censure, that we have sometimes feared that it might be thought that we were rather impelled by personal pique than a proper regard for the interests of science; and yet the author is personally unknown to us, and we have only been led, if we know ourselves, to deal thus freely with his opinions, because we apprehend that his talents, his learning, and the eminent station which he occupies were calculated to disseminate his unsound doctrines over a widely extended country. When we next hear from him, we hope to have more to commend, and an opportunity of aiding him to propagate sound and rational views of a science so nearly allied with the best interests of society.

C. D.

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Art. XIII. *The Pharmacopœia of the United States of America.* By authority of the National Medical Convention, held at Washington, A. D. 1830. Philadelphia, John Grigg, 1831.

*The Pharmacopœia of the United States of America.* By authority of the General Convention for the formation of the American Pharmacopœia, held in 1830. New York, S. Converse, 1830.

If the medical and scientific world were restricted to the most simple modes of expression and inter-communication, if we possessed, for example, but one nosology, but one system of natural history, but one language of chemistry and pharmacy, it is obvious that the books which treat of those sciences, would be greatly simplified; that the labour of learners would be abridged, and much confusion prevented among those who respectively teach, or cultivate, these departments of knowledge. Of this fact, the public are so well aware, that attempts have been many times made to establish in these sciences, standards of definite expression. Sometimes under the sanction of governments, sometimes from the influence of popular writers or teachers in science, and sometimes from the conventional authority of delegated bodies, a common language has been introduced, and obtained a degree of currency, which though seldom universal, has, nevertheless, been sufficiently extensive to produce a full proof and conviction of its utility.